LETTERS TO THE EDITOR.

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The Advancement of Science.

SOME weeks ago Sir Oliver Lodge directed attention to the congestion of work in Section A of the British Association at Leicester. Last week Dr. Chree remarked upon the comparative neglect on our part of such scientific subjects as terrestrial magnetism and the local variations of gravity, which cannot be pursued adequately within the walls of a laboratory, but depend upon observations "in the field." In the meantime, NATURE has noted Dr. Mill's protest against the scanty opportunity for the discussion of meteorological subjects at Leicester.

My own experience at Leicester supports Dr. Mill's protest. I do not refer to what happened to my own paper. I have no complaint to make against the officers of the section, who, with the rest of us, were victims of an unworkable system. I refer to the proceedings with regard to the papers by Mr. Petavel and his fellow-workers at Manchester on the investigation of the upper air.

Since the meeting, I have learned that the results of the work of the international week at Manchester (the last week in July) were of remarkable interest as showing in an exceptional manner all the characteristic features of the variation of temperature in the atmosphere up to a height of 20 kilometres. The papers were among a large number on the programme for Tuesday, August 6. When I left the section at three o'clock to attend a conference as the delegate of Section A, they had not been reached. When I returned at half-past four I was told that the proceedings of the section for the year had already been concluded, with the usual votes of thanks. Whether the papers had been read in the interval or withdrawn I do not know, nor is it of much consequence. If the only time to be found for a subject of such general interest is after three o'clock on Tuesday afternoon, it is clear that some change is required.

The sectional proceedings on Tuesday opened with a discussion upon new methods of treating observations, an important practical matter for the observational sciences. In the circumstances, it was evidently desirable that the opening paper should be printed *in extenso*; but the recorder pointed out to me, and quite rightly, that such a proposition could not be entertained by the British Association, because the committee of Section A had adjourned for the year on the previous day.

These things do not make for the advancement of science.

I wish, however, to take up the point raised by Dr. Chree, and to emphasise the fact, already too obvious to those who have to do with such things, that subjects like terrestrial magnetism, seismology, atmospheric electricity, and the physics of the globe generally, without any reference to meteorology in particular, suffer very seriously in this country from the congestion of work in Section A.

On the one side, work is done of which the scientific public know little or nothing. Atmospheric electricity is a flourishing study on the Continent; seismology is now the subject of an international organisation with Government support; terrestrial magnetism has called for expenditure on a large scale for an establishment to replace Kew as the normal observatory. It is desirable that the association should know what is going on in such matters.

There are, moreover, a number of departments of Government the work of which has at least its scientific side. Papers of scientific interest could probably be obtained for the asking, from a number of competent workers, by an energetic president or secretary, animated by the meritorious wish to use the meeting of the association to bring the scientific staff of the various departments into touch with the scientific public; but the officials in charge of such work have not the advantage of academic long vacations. The time spent at the British Association must be taken either from short leave or from duty. The matter must, therefore, be treated in a business-like way, which in present circumstances is impossible.

It would be absurd, for example, for a secretary or an organising committee to ask, let us say, the hydrographer of the Navy for a paper on submarine centres of magnetic disturbance, or the Astronomer Royal for a paper on magnetic storms and sun-spots, or any other aspect of the magnetic or meteorological work of the Royal Observatory. There is the paralysing consciousness that the time for reading the papers would have to be looked for in a general scramble between three and half-past four on Tuesday afternoon. What is true with regard to these distinguished public servants is equally true with regard to distinguished foreign workers in science.

There is provision in the constitution of the association for asking competent persons to prepare reports upon recent progress in particular branches of science. The provision is, unfortunately, a dead letter in the subjects mentioned. One reason at least is obvious—there is no time to listen to such reports, however valuable they might be.

It is time that we recognised that the attempt to include in one section with mathematics subjects like laboratory physics, in which workers are many and in constant intercommunication, and subjects like terrestrial magnetism, atmospheric electricity, and other branches of geophysics, in which workers are few and widely scattered, is disastrous for the one class of subjects; and, judging by the way in which a discussion upon so important a subject as the measurement of temperature by radiation was received at Leicester, it is not too successful for the other class.

Some years ago there used to be a subsection for the outdoor subjects, with the not very euphonious title of "Astronomy and Cosmical Physics"—perhaps astronomy and geophysics might be better. It has disappeared—not on account of any want of success while it lasted. It was simply omitted from the South African arrangements. The circumstances which called it into existence have now become more pressing. Laboratory physics has become more radio-active, and the other subjects have extended their operations. The temporary expedient of a special subsection is not now adequate. One special secretary at least is required in the interest of those branches of geophysics which are not covered by astronomy. The occasional treatment of such subjects in a presidential address would be of real advantage to science in this country.

I ask, therefore, the hospitality of the columns of NATURE in order to appeal, in the name of the advancement of science, for the establishment of an independent section of the British Association which shall have sufficient time at its disposal to promote the advancement, not only of meteorology, but also of such subjects as terrestrial magnetism, atmospheric electricity, seismology, and geophysics generally. The briefest consideration of the changes which have taken place since Section A was initiated will show that such an appeal is not unreasonable.

W. N. SHAW.

October 12.

On Correlation and the Methods of Modern Statistics.

I REGRET that the pressure of work associated with the opening of a new session did not permit of my replying to Mr. Hinks last week. His letter (October 3) is so far satisfactory that it gives evidence that one professional astronomer realises the existence of stellar correlation; but Mr. Hinks will have to advance much beyond "scatter diagrams" before he can hope to get much profit out of modern methods. Further, may I suggest that he would be more just to both Miss Gibson and myself if (1) he read her paper carefully, and (2) he did not suppose that, because we approach the subject from a different standpoint from himself, we are of necessity both very ignorant and very foolish?

At the expense of reiteration, I must again refer to one or two facts. There are, in my opinion, three points of much interest in Miss Gibson's memoir:—

(*a*) The correlation of magnitude and parallax is shown to be low; what correlation exists is shown to depend

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largely on the sinuosities of the regression curve, and not on a uniform decrease of parallax with magnitude. Miss Gibson's values for the seventy-two Newcomb stars are:—Correlation coefficient, $+0.1 \pm 0.1$; Correlation ratio, 0.5 ± 0.1. For the 173 stars given in the Yale memoir, Dr. Lee has shown that Correlation ratio $= 0.28 \pm 0.06$.

Now let us see how Mr. Hinks faces such results. In the report of the British Association discussion, published with his approval in the Royal Statistical Society's Journal, he refers, in the *first place*, to the theoretical question, namely, he speaks of the theoretical possibility that the relation between luminosity and distance is controlled by a logarithmic curve, and makes the suggestion that this curve (although referred to several times in Miss Gibson's memoir) had been overlooked by us, and that accordingly she ought not to have used the correlation coefficient, which might screen under such a value as 0·3 the really perfect correlation which would flow from the logarithmic relation. What was the meaning of Mr. Hinks's appeal to the logarithmic curve if he did not at the British Association suppose Miss Gibson's value of the relationship of magnitude and parallax to be an underestimation? If that appeal was made to show that she ought to have used the correlation ratio, then he had clearly not studied her paper before criticising it. The charge made at the British Association is indirectly repeated in the last words of his letter in NATURE, where he talks about the propriety of calculating the correlation ratio, as if it had not actually been given on p. 452 of the memoir. The divergence between the correlation coefficient and the correlation ratio shows the trained statistician that the sinuosities of the parallax-magnitude curve are not solely humps due to random sampling.

Well, let us come to our one point of agreement at present: "Astronomers do not believe that magnitude is closely related to parallax." I am glad Mr. Hinks accepts this view, and I will refrain from quoting the work of some great astronomers to show that it has not always been their opinion. Mr. Hinks states in his letter that I asked for details of the reason for the switchback character of Miss Gibson's diagram of parallax and magnitude. If he will read my letter carefully, he will find I asked for no such thing. I asked for details of *his* reasoning at the British Association that the dips and humps were produced by selection of proper motions, which is an entirely different point.

He said at Leicester:—"The second peak belongs to stars of the fourth and fifth magnitude; they were not representative of the average star of that magnitude, but had been chosen *because of their exceptionally large proper motion.*" I very pertinently asked him why this selection had not also been applied to stars of the second to third and to stars of the sixth magnitudes, and further to demonstrate that if it had been thus applied it could possibly have produced the desired effect. To produce an effect on the correlation of A and B by selecting C, a third character, A and B must both be fairly highly correlated with C, and, further, to produce humps we must show that the selection was concentrated at certain points of the range. Mr. Hinks, to give a logical reply, must therefore show:—(1) that parallax and proper motion are highly correlated; (2) that proper motion and magnitude are highly correlated; (3) that the selection of astronomers has been discontinuous along the magnitude range. Instead of proving (3), Mr. Hinks has pointed out that the "humps" in the curve are due to the presence of individual stars of low or high parallax in the special groups, or rather that *some* of them are. Quite so; any statistician knows that with a population of seventy-two the averages of eleven subclasses will be largely influenced by individuals; but the statistician calls this a result of *random sampling*, and does not suggest discontinuous selection by a third variable with a relatively low correlation to at least one of the two characters. Did Miss Gibson, however, lay any special stress on these humps? On the contrary, she says:—"It is possible that a curve of a somewhat complex character—a quartic curve, for instance—might fit the observations." But she concludes:—"Examining Fig. 1, we see that on the present data nothing better than a horizontal straight line at the mean parallax, or a zero correlation coefficient is likely to be found to fit the observations." I think this will show that we laid no special weight on the humps. On the other hand, the high value of the correlation ratio compared with the value of the correlation coefficient does suffice to suggest that astronomers should be cautious about assuming even a moderately low, but continuously descending relationship between luminosity and distance. The rise in parallax of the faintest stars in Miss Gibson's diagram is again manifest in the Yale results, and is probably not due to mere random sampling. The thirty-six stars of magnitude eight to nine in the Yale data have a parallax nearly three times as great as those of magnitude six to seven, which number thirty-one. Will Mr. Hinks assert here again that the former group have been selected by proper motion and the latter have not?

Let me further remind him that the correlation between magnitude and proper motion has not even been mentioned by him in his argument, and yet this vital relationship, for which I have further determinations, is lower than 0·2. Accordingly, it would need a very high relationship between parallax and proper motion to reduce by a proper motion selection the magnitude-parallax relationship to a small value.

(*b*) The second point in Miss Gibson's memoir was involved in the statement that the correlation between parallax and proper motion was "quite significant and important, but not half-way up the scale of correlation" (p. 449). Mr. Hinks says that I am misled by Miss Gibson's results, and asserts that his scatter diagram shows "considerable correlation." Mr. Hinks is an astronomer, and therefore knows the value of exact numerical work. Miss Gibson obtained in her memoir the value 0·4 for the proper motion components. Does he really suppose that his scatter diagram can demonstrate that the value of the whole proper motion correlation is greater or less than this?[1] My estimate, however, was based, unfortunately for Mr. Hinks, not on Miss Gibson's results, but on determinations of the correlations of proper motion and parallax, which involve not only component, but total proper motion. Actually the Newcomb stars exaggerate the result, and the relation between the two characters is sensibly under Miss Gibson's value. Thus for the Yale stars, when we deal with the *whole* proper motion, it is only 0·36. Mr. Hinks says that Miss Gibson "has naturally obtained a smaller result than if she had used the whole proper motion in seconds of the arc on a great circle as is usually done." If Mr. Hinks had studied the subject of correlation a little more fully, he would have known that one correlation is related to the others, and if he had actually worked out the theoretical relationship between them he would not have attributed the lowness of our estimate of the parallax and proper motion correlation to dealing with component instead of total proper motions. The multiplying factor is, I think, $\sqrt{2/(1+r)}$, where r is the mutual correlation of the components $= 0.3$.

Mr. Hinks would support his view of high relationship between parallax and proper motion by *selecting* seventeen stars by magnitude, and leaving the reader to form a mental impression of their correlation. He asserted that our results on seventy-two stars were vitiated by selecting by proper motion, yet he does not hesitate himself to select, not seventy-two, but seventeen stars by magnitude. And what is the result even of this selection? Why, that 40 per cent. of the value he reaches for correlation depends upon the fact that he has not sufficiently counteracted the overwhelming influence due to his inclusion of α Centauri in the seventeen! If he leaves this star out (or reduces its influence by introducing another seventeen stars) the value of the correlation differs from Miss Gibson's value by less than the probable error of the difference.

I must now state the conclusion to which I feel myself driven, namely, that astronomers in the near future will not suppose a *very* close, but a "quite significant and important" relationship between proper motion and parallax. The relationship is more intense than that of parallax and magnitude, but, as shown by the Yale data, it is probably less than the value originally fixed by Miss Gibson, and I

[1] For the total proper motion the Newcomb stars give 0.58 ± 0.05.

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hold that this relatively low value is the second point her paper has indicated for the first time. Meanwhile, I leave Mr. Hinks to consider whether the proven correlations of both proper motion and magnitude with parallax (under 0·4) have or have not any significant bearing upon the differential method of determining parallax, and upon the fact that more than 20 per cent. of negative parallaxes can be found.

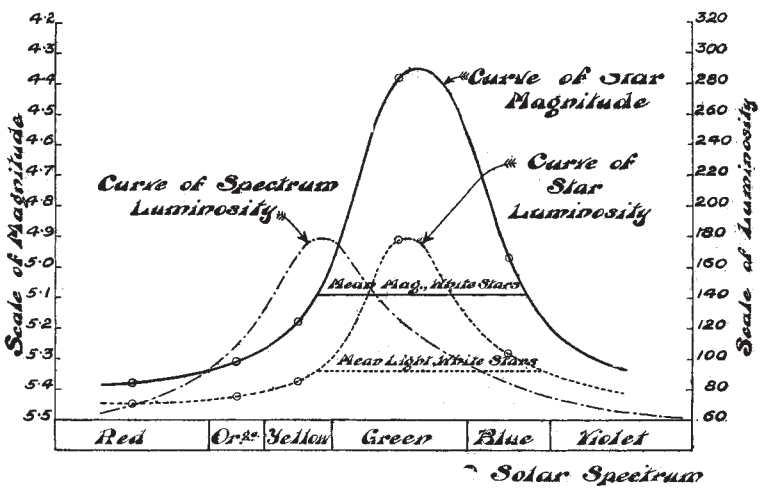
(*c*) The third point in Miss Gibson's paper was the statement that colours (and probably spectral classes) were more highly correlated with magnitude than distance. Mr. Hinks takes this point as one which will fully justify his criticisms at Leicester. I am of opinion that it is peculiarly a case in which he would have done well to have tempered his judgment by previously asking whether there was no method in our madness. He charges us with three grievous offences:—(1) using a highly selected material; (2) omitting to take into consideration the "white stars"; and (3) deducing from such material sweeping conclusions about the stars in general. He further charges me, *on the basis of this investigation*, with asserting "that colour and magnitude are related at least as closely as parallax or proper motion and magnitude."

In my letter, when making my statement, I made no reference to Miss Gibson's published work, but the fact that I cited the value of the correlation of magnitude and spectral class which is *not* given in the published paper might have warned Mr. Hinks that we held other reductions in our hands. Mr. Hinks asserts that our results would have been modified had we included the "white" stars. Using the 2834 stars of the Harvard Catalogue, of which roughly one-quarter are white ("Annals," vol. xiv.), Miss Gibson worked out more than a year ago the contingency of colour and magnitude; the value was 0·27 (±0·01), as compared with the 0·30 (±0·05) of the list in the Cape Observatory "Annals" previously given by her. Omitting the white stars from the Harvard data, the value is 0·297, agreeing absolutely with the result obtained from the Cape data. Thus we see that Mr. Hinks's suggestion that the Cape Catalogue is worthless, owing to selection of special stars, has no validity at all when we turn to the relationship of colour and magnitude, and, further, the inclusion of white stars produces, as we had logically anticipated, no sensible effect.

But I will go a step further, and reveal another conclusion, which I should naturally have preserved for the present, as the research is as yet incomplete. The mean magnitude of the white stars is almost identical with the mean magnitude of all the remaining truly "coloured" stars; the white star has not a preponderance of any special part of the colour spectrum, and if we wish to investigate the relationship between luminosity and colour we must logically leave out the white stars. The accompanying diagram gives the Harvard stars classed according to colour, with (*a*) the mean magnitudes of each colour group, and (*b*) the corresponding luminosity on the assumption that the light of a tenth-magnitude star is unity. It will be seen that the stellar luminosities form a curve very similar to the light-intensity curve of the solar spectrum, but shifted towards the violet end of that spectrum, possibly owing to the fact that the average star is hotter than our sun. On this scale there is clearly no place for the white stars, and the essential feature is that stellar magnitude takes its place in a continuous and definite relation to stellar colour.

I had no intention of anticipating work not yet completed, but Mr. Hinks's contemptuous reference to our omission of the white stars needed to be dealt with. Their inclusion or exclusion makes no difference from the standpoint of the statistical constant; their exclusion is, however, justified by the physical considerations which I have here suggested.

I should wish to say one word, albeit I am afraid it must be a strong one, about Mr. Hinks's further treatment of Miss Gibson and myself. In the paper, to use its own words, a suggestion, "even if it be only of the *vaguest* kind," is made that the bulk of the lucid stars *may* belong to a differentiated system. Mr. Hinks asserts that the basis for "this far-reaching suggestion" is one in which the white stars had no frequency in the record. Will the reader believe that this suggestion, which the writer herself describes as of the "vaguest kind," is not based on the colour correlation at all? Can Mr. Hinks really have criticised the memoir and supposed that even this vague suggestion was based on the 159 Cape stars? The suggestion, such as it is, is based on counts of *all* stars, and results from showing that a continuous curve can be found which describes with remarkable closeness the counts up to the sixth and seventh magnitude, but that beyond this magnitude any formula hitherto proposed fails even approximately to describe the frequency. This result, reached by other investigators, is confirmed by Miss Gibson, and in association with changes in other stellar characters, which occur about the same magnitude, does suggest, I venture to think, that in the vaguest kind of way some differentiation of the stellar system may possibly exist beyond the bulk of the lucid stars. I think Mr. Hinks owes us an explanation of what his statement, that a far-reaching suggestion has been based on stellar statistics from which all white stars have been excluded, really is intended to convey.



In conclusion, may I say that I have learnt from my experience with biologists, craniologists, meteorologists, and medical men (who now occasionally visit the biometricians by night!) that the first introduction of modern statistical methods into an old science by the layman is met with characteristic scorn; but I have lived to see many of them tacitly adopting the very processes they began by contemning. Mr. Hinks is at present in the first stage; but may I remind him that even astronomy owes something to the layman, and express my hope that he may quickly reach a more understanding and sympathetic frame of mind? KARL PEARSON.

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The Body of Queen Tii.

JUDGING from the letter addressed to NATURE of September 26 (p. 545), Mr. Hall (like Prof. Sayce in the *Times* of September 17) has been thrown into a state of doubt in regard to the real sex (? and age) of the mummy supposed to be "Queen Tii" by a letter from Mr. Theodore Davis, calling in question the accuracy of my statement that the mummy supposed to be an old lady of at least fifty years is the skeleton of a young man of about half that age.

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